strips till one or two seconds after the exposure had been made. If, however, the time of swing of the galvanometer was larger than 5 seconds—10 seconds, for instance—the curves show that the error would increase nearly proportionally to the time of swing of the galvanometer.

I am not aware what was the time of swing of the galvanometer which Dr. Ångström employed in his comparisons, but I suppose it to have been of the order of 3 or 4 seconds, which is that which we customarily employ in pyranometer observations. If this is the case, I am of the opinion that it is quite impossible that error of the order of 5 or 6 per cent, such as he calls attention to, could have been due to this source. The tendency of the error is, of course, to make our instrument read too low. Dr. Ångström does not say in what direction the discrepancy between the two instrument lies.

FORECASTING THE WEATHER ON SHORT-PERIOD SOLAR VARIATIONS.

By CHARLES F. MARVIN, Chief U. S. Weather Bureau.

[Washington, D. C., April 4, 1920.]

In the remarkable paper [1] cited below, Mr. Clayton claims he has established important relations between high and low values of the day-to-day intensities of solar radiation, E_o', as measured by the Smithsonian Institution, chiefly at Mount Wilson, Calif., and Calama, Chile, and the values of the mean temperature at Buenos Aires. By means of these relations he claims material improvements in forecasting the weather are made possible. These investigations are an extension of earlier studies by which this author [2] endeavored to show that the whole earth responds in a complex but definite manner to the small changes of a few per cent in the reduced values of solar radiation as measured at Mount Wilson, Calif.

The forecasting value and possibilities of knowledge such as Mr. Clayton claims to have disclosed is obviously very great and important *provided his claims are true*. The writer, however, quickly became firmly convinced, purely from basic principles, that Mr. Clayton, who seems to regard the day-to-day changes in the observed values of solar intensity are mostly of solar origin, is quite in error. Indeed, great harm is being done to the cause of weather forecasting and the real progress of science by the wide dissemination of unrefuted representations of this character.

The whole matter seems to the writer to be a case of the seemingly complete disregard in the discussion of data of the material errors of observations and of the laws and operations of chance. Such a course has necessarily resulted in a grave misinterpretation of an excellent mass of observational data. Urged by these convictions, the writer has endeavored to evaluate, if possible, the unavoidable random and partially known dominant errors of measurements of solar radiation. This study was approached with a full belief in some solar variability. The results, however, unequivocally show that the observed changes in day-to-day values of radiation are very largely due to the aggregate of all the unavoidable sources of error of determination, all wholly terrestrial. The possible frequent and irregular variations of solar intensity from day to day or over an interval of a few days must be quantitatively such a small fraction of 1 per cent that it can not be satisfactorily evaluated from the existing data even including those now being secured by the new pyranometric method of observation. Such variations, if any actually occur, must be so small as to be quite inconsequential as a controlling factor of the weather and temperature of to-morrow or the next few days at any particular locality.

The only question the writer discusses in this paper is the *changes* of intensity from *day-to-day* or from some daily value of intensity to the next daily value observed a few days later. These are the variations in observed values which Mr. Clayton has used as the basis of correlation between solar intensities and temperature changes at Buenos Aires.

The writer particularly desires to avoid making any statement either for or against slow long-period solar changes, that is, changes over a few weeks, months, seasons or years, for example. He distinctly desires to leave open the question of regular or irregular changes of this character. The manner in which terrestrial weather responds to such changes can not be intelligently discussed until such changes have been conclusively shown to occur and been at least fairly evaluated in amount. An investigation with this object in view is also in progress.

The real question now at issue is simply the *variability* of daily or quite frequent *observed* values of solar intensity outside the earth's atmosphere, and how much, if any, of this variation is caused by true solar changes and how much caused by errors of measurement and varying depletions of large masses of the atmosphere which transmit all incoming radiation before measurement of its intensity.

Seemingly, one of the most direct, if not the best methods of solving such a problem consists of a critical evaluation, by means of well-known statistical methods, of the *variation* of the observational data of which an excellent body of over 1,500 frequent values of intensity is now available.

Within the past few weeks the writer has made a somewhat hasty preliminary review of these data, and it seems proper to briefly mention in this preliminary note certain important facts which seem to stand out unequivocally.

(1) The frequency distribution of the data is nearly Gaussian, that is, it nearly conforms to the normal error curve of statistics. Therefore, the data may be discussed by the methods of least squares.

(2) The distribution is not entirely elemental, but in this feature it reflects and justifies the composite make-up which the theory of the variations as expressed in equation (1) below calls for.

(3) There is only slight skewness in the distributions, which varies a little in amount and kind (positive or negative) according to the particular group of data analyzed. The evidence from skewness justifies the assertion that for observations at Mt. Wilson, Calif. by the bolographic method and on the average, *changes* in transmission of the atmosphere during observations tend to give a preponderance of slightly too low values of intensity and correspondingly too high values of the coefficient of atmospheric transmission.

(4) *Changes* of transmission during observations also cause greater scattering and dispersion of values than would otherwise occur, thus imposing upon the data many false variations due entirely to atmospheric, not solar, causes.

[1] Variation in Solar Radiation and the Weather, by H. Helm Clayton. Published simultaneously in Spanish in the Boletin Mensual Oficina Meteorologica Argentina, and in English in Smithsonian Miscellaneous Collections, vol. 71, No. 3.
[2] Smithsonian Miscellaneous Collections, vol. 68, No. 3.

(5) *Values of E_0 versus E_0'.*—The Astrophysical Observatory publishes two values of solar radiation intensity, designated, respectively, the reduced value E_0 and the final value E_0'. The latter is derived from the former by the application of a secondary correction for water vapor found to be necessary because of a certain correlation where none should exist between E_0 and weather and sky conditions at Mount Wilson. The values E_0 are derived from the rigorous reduction of the observations according to the Langley-Bouguer theory. The secondary water-vapor correction is always additive and increases the value of E_0 in the long run, and by trial it is found the correction does not reduce the scattering or dispersion of the values of E_0 as is required by the theory. The writer considers the values of E_0 most appropriate for fundamental studies of solar intensity data. Mr. Clayton has used only values of E_0', but one value is just as good as the other for his particular studies.

(6) The principles of physics and laws of errors lead to the following basic equation:

$$\sigma = \sqrt{\frac{\Sigma v^2}{n}} = \pm\sqrt{s^2+\alpha^2+i^2} \qquad (1)$$

in which σ is the gross standard deviation of the data due to all causes, that is σ is the semiparameter of the Gaussian curve of best fit to any group of n observations which furnish the residuals Σv^2; s is the part of the deviation caused by solar changes, if any exist; α is the measure of the variations due to purely atmospheric effects or controls, and i is the part of the measure of gross variation attributable to instrumental errors. The latter is the measure of errors caused by operations *within the observatory* as contrasted with α which arises only by what happens to the incoming radiation during its transmission through the changing air mass. These two latter causes of variations are wholly terrestrial and can and often do exercise a powerful terrestrial control on a single value of radiation intensity.

Everyone will admit, we believe, that material and finite values must be assigned to α and i before we can speak in quantitative terms of solar variability, no matter what method of observation is employed. Seemingly, no adequate effort to do this has yet been made. It is clearly most improper to assume that α and i are wholly negligible in single values of intensity, yet, in effect, this is necessarily done when the day-to-day *observed* values of radiation (or even the average of several values selected because they are extreme) are used in establishing correlations between *alleged* solar changes and terrestrial weather. Results thus secured are clearly dependent upon the errors of measurement represented by the quantities α and i, and must therefore be distrusted.

If E_0 is the mean value, and if σ is the standard deviation of a group of n observations, then by least squares the probable error or probable variation, ϵ, of a single daily value in percentage amount will be

$$\epsilon = \pm\frac{0.6745}{E_0}\sqrt{\frac{n\sigma^2}{(n-1)}} = \frac{0.6745}{E_0}\,\sigma \text{ if } n \text{ is large.} \qquad (2)$$

Now a gross value of ϵ resulting from *all* causes of variation is easily computed from the data. For good and bad observations, as a whole, it is found the value of ϵ rarely exceeds ± 1.3 per cent for bolographic observations, at Mount Wilson, Calif.[3] whereas taking good and excellent observations ϵ falls generally below 1 per cent, and Dr. Abbot's new method of observation by means of the pyranometer in use at Calama, Chile, yields unselected data (292 values) which show a gross probable error of only 0.79 per cent, and less in later data. The sky conditions at this station appear to be much superior for solar work to those at Mount Wilson, and the gross probable error of a single value by the bolographic method at this station was only 0.94 per cent for all observations (247) July, 1918 to July, 1919.

The smallness of these quantities alone signifies high intrinsic merit in the observational data and furnishes a very narrow margin for frequent and irregular short-period solar variations. In fact, the solar radiation investigations conducted by Dr. Abbot constitute a monumental research of the highest possible order and command only the admiration of all. The representations made in this note have to do only with a question of the entire correctness of the interpretation put upon the irregular day-to-day and short-period changes in observed values.

There is no evidence in Mr. Clayton's paper that the inherent errors of observations of solar radiation have been evaluated by him or considered in any way, and the suspicion is justified that the whole fabric of his correlations rests very largely upon the errors caused by terrestrial controls in making observations of solar radiation at Mount Wilson. The writer has no comment to offer here on a claim which may be made that Mr. Clayton's work at least proves that there is a correlation between Buenos Aires temperatures and the undefined atmospheric states at Mount Wilson associated with extremes of solar intensities.

This preliminary note is submitted at this time to justify and support certain adverse criticisms made by the writer in the discussion of the paper read by Dr. Abbot at the meeting of the Washington Academy of Sciences[4] January 29, 1920, presenting the results of Mr. Clayton's studies.

The whole question of short and long period solar variability, and the terrestrial response thereto in terms of weather, is obviously one of great importance to applied meteorology and to science in general. It is very necessary, therefore, that the splendid observational work done by the Astrophysical Observatory be generously supported and extended, in order that the outstanding and unsettled questions of the correct interpretation of the observations may be brought to a conclusion with which all students may agree.

A more detailed statement of the grounds for the writer's views and the statistical basis for his conclusions is in course of preparation and will be offered in the near future.

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[3] Dr. Abbot has kindly supplied me in advance of publication with manuscript copies of all observations made at Mount Wilson during the years 1912 to 1918, inclusive. These are thankfully acknowledged.

[4] Journal of the Washington Academy of Sciences, April, 1920, vol. 10, No. 8, pp. 226 to 236.